permanent vertebræ. The new segmentation, however, does not follow the lines of the earlier division, but passes between the ganglionic and the vertebral portions, in fact, through the middle of each protovertebra. In consequence, each spinal ganglion and nerve ceases to form the front portion of the primary vertebra formed out of the same protovertebra as itself, but is attached to the hind part of the permanent vertebra immediately preceding. Similarly, the rudiment of each vertebral arch covering in the neural tube, no longer springs from the hind part of the protovertebra from which it is an outgrowth, but forms the front part of the permanent vertebra, to which it henceforward belongs. . . . . By these changes this remarkable result is brought about, that each permanent vertebra is formed out of portions of two consecutive protovertebræ."

Such being the case, the question suggests itself as to what becomes of the portion of the new column corresponding to the anterior or cephalic end of the protovertebræ nearest the skull which has no other semivertebra wherewith to blend. It has no neural arch, and does not enter into the formation of the cranium, for the protovertebra does not enter that complicated structure. We are not informed as to its destination. May it not be that it persists as the odontoid process of the axis, which, from not being able to maintain an independent existence, joins, late as we know, the second cervical vertebra? This hypothesis involves a difficulty, no doubt, as to the nature of the atlas, but seems to throw some light on the peculiarity in the conformation of the axis.

The development of the vascular system, through the various complex stages, is most fully explained, with the assistance of several very instructive diagrams. We can hardly help having a feeling of regret that the common fowl does not resemble its allies, the Mound-makers (*Megapodidæ*), in having only a single carotid artery instead of two, because then we should have the question answered as to the method by which the companion vessel is lost, which is at present not in the least understood.

We must refer our readers to the work itself for an account of points so important as the development of the Wolffian bodies and their ducts, the spinal cord, the heart, the nasal pits, as well as the many other details respecting the different organs which go to form the adult bird; original observations will be found on most; and where these are absent, the excellence of the *résumé* of the work of others will clearly prove with what conscientious care the authors are carrying out the evidently pleasurable labour they have imposed upon themselves. As far as the permanent kidneys are concerned, it must be mentioned that, from their manner of development, it is shown that their separation morphologically from the Wolffian bodies is an occurrence of *purely secondary importance*.

Now that we have a text-book of embryology produced under such favourable auspices, it is to be hoped that the far-spread ignorance on that subject, which is at present but too apparent on all sides, will no longer exist, and that a higher standard whereon to commence further investigation will quickly develop itself amongst all English students of biology.

A. H. G.

OUR BOOK SHELF

A Monograph of the Post-tertiary Entomostraca of Scotland. By G. S. Brady, H. W. Crosskey, and D. Robertson. (Palæontographical Society.)

In this part of its publications the Palæontographical Society has done good service to that large body of geologists who take interest in the story of the old glaciers and icebergs of Britain and love to gather when they can the traces of the life which peopled the frigid sea once spread over some of the richest tracts of our present islands. The descriptions here given of the localities and sections of the glacial deposits are perhaps all that could be at present attempted, but they offer a very puzzling problem to the reader who would fain know something of the chronology of the deposits. Nothing can show more satisfactorily the labour which has in recent years been bestowed upon these Post-tertiary clays than the fact that a few years ago not one of the minuter forms of Crustacean life had been noted as occurring in them, while now more than 130 species belonging to twenty-seven genera of Entomostraca have been carefully examined and described by the authors of this Monograph. The names of Brady and Robertson are a sufficient guarantee for the faithfulness of these descriptions, while Mr. Crosskey's knowledge of the localities and his expertness as a collector have given an additional fulness and value to the Monograph. Though but dry reading for ordinary people, these pages, with their accompanying admirably executed plates, will be a valuable boon to many a student of Post-tertiary geology.

The Races of Mankind: being a Popular Description of the Characteristics, Manners, and Customs of the Principal Varieties of the Human Family. By Robert Brown, M.A., &c. Vol. II. (London: Cassell, Petter, and Galpin. *No date.*)

This work, which seemed in the first volume to promise some scientific value, is now down to the level of the popular picture-book. There are some good pictures in it, and no doubt the boys and girls who have it given them will pick up ideas from its compiled information. We have not done more than look into it here and there, finding errors small and great. At p. 4, a Spanish-American is called, with curious felicity of blundering, "Don Jose Marie del Muchos Dolores." At p. 22 we read, "The smallest shopkeeper in Germany expects to be addressed as Mr. Court-Councillor." At p. 284 is a picture of a Bushman playing on the goura, or musical bow. This appears to be an altered copy of the illustration in Mr. J. G. Wood's "Natural History of Man," vol. i. p. 295; but whereas Mr. Wood's artist knew that Bushmen have narrow heads, and drew his accordingly, the present draughtsman, by his alteration, has given his native a skull of enormous width. At p. 113, in contradiction to the weight of ethnological evidence, the Australians and Tasmanians are treated as belonging to the same race. When we add that many of the illustrations are taken without acknowledgment from Figuier's "Human Races," it is not necessary to inquire further where M. Figuier got them.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

The Royal Agricultural Society and the Potato Disease

My main object in writing to you was to correct what Mr. Jenkins admits were "grotesque statements," and to claim for a distinguished English botanist credit for work done by him thirty years ago, which I was unwilling, without protest, to see assigned to anyone else.

© 1874 Nature Publishing Group

No doubt the Royal Agricultural Society was not founded for the advancement of science in general or of botany in particular. When, however, it transcends the practical limits it has imposed upon itself, and promotes a purely scientific investigation, the way it sets about it is, I suppose, a fair object of criticism in a scientific journal.

Mr. Jenkins complains that I have not taken the trouble to read the official reports published by the Society, and thinks my criticisms upon them might have had some value. As a matter of fact I have done so, and my difficulty is to be quite sure that I understand what the last and most important really means. To say nothing of the occurrence of "jonidia" for "conidia," I find the following sentence:—"Prof. de Bary expresses sanguine hopes that he has at last discovered the certain nids (*sic*) or resting-places of the oospores or active primary germs of the fungus." It would not have occurred to me to describe oospores—in other words, resting-spores—as *active*, and it has been suggested to me as not impossible that oospore may also be a misprint in place of zoospore. There is the more necessity for caution in the matter, as the publications of the Royal Agricultural Society do not seem to receive the botanical revision that might have been expected. Only last year—and it was not a solitary blunder—a fungus was figured in the Society's Journal as Aspergillum (*sic*), which was obviously no Aspergillus at all, but the common Bread-mould (*Ascophora Mucedo*). No doubt, in due course, we shall have the opportunity of reading, at full length, what Prof. de Bary has added to our knowledge of the matter; but in the meantime we should not forget what is due to those who have already worked at the subject in this country.

Mr. Jenkins denies that the Society offered prizes for disease-proof potatoes. I find that in the report of the judges on the abortive essay competition, presented to the Council, Dec. 10, 1873, it was recommended, "That valuable prizes be offered for (*a*) The best disease-proof early potato; (*b*) The best disease-proof late potato." Again, the recently published official report to which I have already referred commences: "The judges appointed to inspect the growth of the six varieties of potatoes, *which were entered for competition as disease-proof*," &c. It may be that this is "colloquial" language, and does not mean what it says; but Mr. Jenkins must know that if by any chance any one of the potatoes tried had run the gauntlet of the three years' trial, it would have been advertised far and wide as stamped with a "disease-proof" character by the Royal Agricultural Society.

Mr. Jenkins complains that I suggest an offensive spirit as actuating the Society in its communications with Prof. de Bary. I can only say that I used the Society's own language. I find that the judges, in their report, *after declining* to recommend any one of the ninety-four essayists for a prize, propose "That a sum of money (say 100*l.*) be granted *for the purpose of inducing* a competent mycologist to undertake the investigation of the life-history of the potato-fungus" (as if nothing had been done in it already). The joint Botanical and Journal Committee thereupon gave notice that they would ask for a grant of 100*l.* to carry out this recommendation. I am not aware that the British Association proceeds in this way in distributing its funds, and I leave Mr. Jenkins to reconcile what I have quoted with his statement, "that the *first* step taken by the Council of the Society was to direct me to write to Prof. de Bary."

Let me sum up the substance of my criticisms. The potato disease has been before the scientific world for thirty years, and has been investigated by Berkeley in England, Montagne and others in France, De Bary in Germany. The Royal Agricultural Society takes charge of a competition which induces ninety-four persons to write on a subject on which it was *à priori* in the last degree improbable that they could have any really important unpublished facts to bring forward within the limits of even the extended time at which the essays were to be sent in. On the failure of this scheme prizes are offered for disease-proof potatoes, "disease-proof" being subsequently defined to mean immunity from disease in twenty different districts for three years. Were a disease-proof potato a probable thing, it might clearly be trusted to establish its own reputation. Lastly, the amateur world of prize essayists having proved fruitless, the cryptogamic botanists of this country—many of them men of European fame, who would doubtless have willingly responded to an appeal from the Council to co-operate in the matter—are passed over *en bloc*, and the matter is placed in the hands of a German scientific man—highly and worthily distinguished, doubtless—but who, I am convinced, would be far from approving the slight placed on our countrymen, one of whom has accomplished what will ever be a classical research in this very subject. I submit that when I applied the expressions "spasmodic," "ill-considered," and "wanting in scientific method" to these proceedings, I was not using inappropriate language.

W. T. THISELTON DYER

Sensitive Flames

PERMIT me to thank Prof. Herschel for his all too kind acknowledgment of the aid my former brief communication to NATURE may have been to him. In a paper on Sensitive Flames that is awaiting the needful leisure to complete, I have given a brief history of this subject—which, by the way, so far as regards the discovery of sensitive flames, Prof. Herschel has partly misapprehended, though there can be no doubt the valuable letters of Prof. Herschel will play an important part in the development of these phenomena. I am glad to find that, so far as Prof. Herschel has recorded his views, they corroborate the results of my own experiments (begun as long ago as 1867) in search of the cause of the sensitiveness of various fluid jets, and the application of sensitive flames to acoustic investigation and other practical ends. For reasons, into which I will not enter here, I was led to postpone this inquiry, and it is only comparatively lately that it has been resumed.

The keynote to the whole of the phenomena is, I believe, to be found in Savart's beautiful investigations on liquid jets. Any fluid body, gaseous as well as liquid, escaping from an orifice in a tranquil stream, consists of a continuous and a discontinuous region, and is subject to the play of opposing forces which excite pulsations in the jet, the number of which is directly proportional to the velocity of the issuing stream, and inversely as the diameter of the orifice. When a note is sounded approximately in unison with the vibration number of these pulsations, the jet of water, smoke, or flame is thrown into more vigorous vibration, and a strained condition of the jet is set up.

Hence it is easy to obtain a series of sensitive flames, issuing from orifices of decreasing size, capable of responding (within a certain range) to the successive notes of the gamut; the higher notes affecting, of course, those flames from the smaller orifices, and which also require to be under greater pressure of gas than the flames responding to the lower notes. The relative rate of vibration of these flames is at once clearly seen by viewing them together in a moving mirror. But I will not weary your readers by further entering upon a subject with which already they must be somewhat tired.

W. F. BARRETT

Royal College of Science, Dublin, Nov. 30

Fossils in "Trap"

I AM much obliged by your insertion of my letter on "Fossils in Trap." You are right in supposing that the trap I referred to was crystalline augitic trap. If it had been tufa I should not have written to you as I did, as I was well aware that fossils in tufa were of common occurrence. Shortly after I wrote I found that the *Favosites gothlandica* which shows the section is still imbedded in a portion of the slate, which is olive-coloured, and closely resembling the trap. This is so intimately connected with the trap that it is impossible to trace a *line* of connection.

Halifax, Nova Scotia, Nov. 14 D. HONEYMAN

[Dr. Honeyman's discovery would appear to resolve itself into the simple fact that his "trap-dyke" has involved in its mass fragments of the fossiliferous strata through which the molten rock has risen—a fact, we presume, with which every practical geologist who has worked amongst igneous rocks must be more or less familiar.—ED.]

THE RELATION OF RACE TO SPECIES

IN a notice of Quetelet's works, published in NATURE, vol. v. p. 358, I raised the question whether this eminent statistician's method of defining a race or population might be applied to provide naturalists with a means of defining species. Since then, the consideration of Mr. Francis Galton's explanatory diagram, given at p. 28 of his work on "Hereditary Genius," has led me to attempt to carry this problem a stage further.

Instead of using, with Quetelet, a binomial curve to show the constitution of a race, with its central type and varieties, Mr. Galton sets before our minds the very indi-

© 1874 Nature Publishing Group